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PROCEEDINGS
OF
THE ROYAL IRISH ACADEMY.

1840.

No. 23.

May 11.

SIR W^M. R. HAMILTON, LL.D., President, in the Chair.

John Davidson, Esq., James Henry Blake, Esq., Q.C.,
and Abraham Abell, Esq., were elected Members of the
Academy.

A paper was read by Jonathan Osborne, M.D., on Aris-
totle's History of Animals :

Dr. Osborne commenced by observing, that this work
was composed under circumstances more favourable to the
acquisition of natural knowledge than any work on the sub-
ject ever published. According to Pliny, some thousands of
men were placed at the disposal of the author, throughout
Greece and Asia,—comprising persons connected with hunt-
ing and fishing, or who had the care of cattle, fish ponds, or
apiaries,—in order that he might obtain information from all
these quarters, *ne quid usquam gentium ignoraretur ab eo*.
And according to Athenæus, the same prince gave him, on
account of the expenses incurred in composing it, 800 ta-
lents,—a sum, which taken at the lowest, that is, the lesser

Attic talent, amounts to above £79,000. The work composed under such auspices, is such as might have been expected. The extent of the observations is prodigious; and we cannot read far in any part of it, without being constrained to exclaim with Cicero, *Quis omnium doctior, quis acutior, quis in rebus vel inveniendis vel judicandis acrior Aristotele?*

Shortly after the introduction of Greek literature to Europe, and when this book was first printed, those sciences which have nature for their object, were in the lowest condition. There was at that time no taste diffused for the study of zoology or comparative anatomy; and at later periods, when the value of these studies came to be better appreciated, the Aristotelian philosophy had fallen into disuse. Thus this work has, from this combination of circumstances, been passed over; is seldom quoted except at second-hand; and no edition of it distinct from the other works of the author, or illustrated as the subject required, has appeared since that of Scaliger, published in 1619,—except one, accompanied by a French translation by Camus, in 1782, which is said to be incorrect, and is become scarce.

Dr. Osborne proceeded to make a short analysis of the contents of this work, and showed that Aristotle had anticipated Dr. Jenner's researches respecting the cuckoo, as also some discoveries with respect to the incubated egg, which have been published within the last year. His observations on fish and cetaceous animals are curious in the extreme, as might be expected from the variety of these animals abounding in the Grecian seas. Those on insects it is difficult to appreciate, from uncertainty as to the names. He describes the economy of bees, as we have it at present; but mistakes the sex of the queen. He holds the doctrine of spontaneous generation in those cases, in which he could not detect the ovary; an inevitable conclusion arising from the want of the microscope, to which, and the want of knowledge of pneumatic chemistry, his principal errors are to be referred.

The various organs are described as modified throughout the different classes of animals, (beginning with Man, the *Βουλευτικον μονον*) in nearly the same order as that afterwards adopted by Cuvier.

As specimens of the interesting matter treated of in the work, Dr. Osborne selected the animal nature of sponges; the ages of various animals; the movements of the nautilus, (the same doubt existing in the author's mind as to the origin of the shell, which has divided the opinions of Messrs. Blainville, Owen, Gray, and Mad. Power, within the last year;) the localities of animals, as affording data for ascertaining the rate at which they have extended themselves over the globe; particulars relating to artificial incubation as practiced in Egypt; the management of cattle; a mode of fattening hogs with rapidity, by commencing with a fast of three days; the mohair goat located in Cilicia, as at present; hybernation and migrations of various animals and fish; description of the fisher-fish (*lophius piscatorius*), and of the torpedo, with the proof that they catch their prey in the extraordinary manner described; many ingenious modes of taking partridge, and of fishing detailed; the friendships which have been perpetuated between different classes of animals,—as the trochilus and the crocodile, the pinna muricata and the cancer pinnotheres, the crow and the heron; their animosities, as between the crow and owl; the diseases of animals traced throughout the series, extending even to fish; hydrophobia described, as being communicated by the bite of the rabid dog to all animals except man, which appears to be the correct statement with respect to hot climates, and not (as has been represented by some modern travellers,) an entire absence of the disease.

These detached specimens of the contents of this work furnish, however, a very inadequate idea of its real value. There are in it whole sections, the separate sentences of which,

would furnish texts for as many Bridgewater Treatises. The freshness and originality of the observations taken from nature herself, and not made up from quotations of preceding writers; the extent of the views, not bounded by any necessity for complying with preconceived or prevalent notions, but capacious as the author's mind itself, and frequently leading the reader into the most interesting under-currents of thought branching off from the great fountain; these are all merits belonging to the work, but not constituting its chief value,—which is, that it is a collection of facts, observed under peculiar advantages, such as have never since occurred, and *that it is at the present day to be consulted for new discoveries.*

Now that Greece is, for the first time since the revival of letters, in possession of a government capable of appreciating scientific investigations, a favourable opportunity offers for preparing an edition of the work, at once worthy of the age in which it was composed, and of that in which we live; and perhaps some individual may be found, possessing a competent knowledge of the Greek language, and of zoology and comparative anatomy, who, after a sufficient examination of the animals now in Greece, shall undertake the task of editing and illustrating this great work. Such a performance, properly executed, would be the resuscitation of a body of knowledge, which has lain buried for above 2000 years; and would certainly be no less acceptable to zoologists and anatomists than to the cultivators of classical learning.

The Rev. Dr. Todd exhibited to the Academy a gold ring, the property of William Farren, Esq., which was received in barter, from the natives of the western coast of Africa. The similarity of the twist in this ring to that of the gold torques found at Tara, and recently presented to the Academy, renders it extremely worthy of attention. The following extract of a letter from Mr. Weston, of Lon-

don, by whom the gold ring was presented to Mr. Farren, and which is addressed to that gentleman, was read to the meeting :—

London, March 31st, 1840.

“ In reference to the African gold, or torque as you call it, a young correspondent of mine, resident at Sierra-Leone, and a merchant there, happens to be at this time in London ; from him and his father I have received many boxes of this pure gold, and he has furnished me with his own information upon the subject, which I send you inclosed ; he tells me there are large rings or torques, full the size of those I saw in the library of the Royal Irish Academy. Recollect the inclosed is written by a man of colour, and an African by birth, educated in England under my care, and now a resident in his own country. By this you will see what some of these men are capable of.

“ I have written to Africa, and desired a large ring may be procured for you ; I have no doubt this discovery may throw some new light, as to the existence of a connexion in early days between Africa and Ireland.”

The following is the letter from the African gentleman alluded to by Mr. Weston :—

“ 31st March, 1840.

“ My dear Sir,

“ In answer to your inquiries relative to the gold rings that are generally sent from Sierra-Leone, I must first candidly explain that what little information I may be able to afford, has been derived from the gold strangers or traders that visit the colony, and not from any personal intimacy with the places where the gold is procured.

“ The gold out of which these rings are *twisted*, is found in the countries of Seral-Doolley, Timbuctoo, Seran-Colley, Follah, Bondou, Kasson, Kaarta, Bambarra, and Timbo, all of which tribes are distant about 1400 miles from, (in latitude 15° N.) and visit Sierra-Leone, in hoards of from two to four and five hundred at a

time, travelling generally on foot ; these journeys take them from two to four months, and equally long to return to their homes. They are all of the Mahomedan persuasion, and proficient Arabic scholars. Their manners are easy and insinuating ; and in conversation, which is always (or generally) done through an interpreter, they are full of compliments and flattery. Agreeably to the Mahomedan creed, they use no liquor, wine, or beer of any kind, (not even ginger beer,) and drink exclusively water, or sugar and water. They are, for the most part, very uncleanly in their habits, and particularly so in their dress—oftentimes wearing one apparel without ever taking it off to cleanse their bodies the whole time they are away from home ; their clothes are consequently almost in rags before they put on new ones.

“ The gold is found in veins, and dug up in a solid substance, resembling the fine roots of trees. It is then purified by a mere melting process, in crucibles, so as to separate the earthy portion from the metal itself. The Africans are not capable of amalgamating the gold, this is left for the refiners in England to do.

“ In some of the countries already mentioned,—Bondou and Timbo, more particularly,—they sweep out their huts every morning, the floors of which are mud ; and no person is permitted to stir out until this office is performed. In the dust they sweep up, a little gold is mixed. They then wash the whole in vessels for the purpose, and the gold naturally sinking to the bottom, is thus separated, and obtained in small quantities. The twisting is accomplished by holding both ends of a solid piece of gold between nippers, and then turning it round until it assumes the appearance in which it is imported, being exceedingly ductile ; this is not a tedious process. The rings thus twisted, are sometimes from *twelve to fifteen inches* in circumference, and weighing about *fourteen ounces*. I however have heard, that they are made much longer and heavier ; but these are not, to my knowledge, parted with in the way of trade, but worn as ornaments round the neck and arms.

“ In the interior, all transactions are carried on for gold, the trader being furnished with a pair of scales made of the hard outer skin of the orange gourd. The weights are the seeds of certain vegetables or fruits. They thus pay in gold from *two pence* to £10, and

upwards. This I have often seen, and proved their exactness by weighing the same pieces in English scales.

“Gold is also found in Central Nigritia, and on the Guinea coast; this I believe is principally in dust, and obtained by the same method of washing.

“I have seen a piece of gold in its natural rough state; it was a solid piece, about five inches long, and of the thickness of a common writing quill. It was smooth in appearance, but seemingly composed of a number of layers, compressed together by a natural mechanical force, with veins like the grain of wood from the root of a tree.

“In the countries I have here alluded to, the natives cultivate farms, but in a very careless and rough manner, merely cutting down the trees, but never rooting up the stumps or clearing away the smaller plants, but plant the rice or cassava negligently among the whole of this stubble, waiting till the rice, &c. may grow, to distinguish one from the other. They never sow or make use of the same farm a second time; but the soil is excessively sterile and sandy. Domestic poultry is plentiful; also sheep, and other horned cattle. They take great care of their cows, milk forming a principal luxury in their daily diet.

“The gold strangers invariably visit Sierra-Leone, accompanied by several slaves, who bring ivory and other articles for barter. They all represent the countries from which they come, as possessing plenty of gold, but no facilities for procuring it.

“Salt is considered a great luxury in the interior of Africa, and eagerly sought after in trading.

“I remain, my dear Sir,

“Yours, assuredly,

“W. GABBIDON.”

The Rev. H. Lloyd, V.P., read the following communication by Dr. Apjohn, on the value of the numerical coefficient, in the formula for the force of aqueous vapour in the atmosphere, as derived from the observations of the wet and dry thermometers.

“If t and t' be the temperatures shown by a dry and wet thermometer, encompassed by atmospherical air, t'' the dew-point, f' and f'' the forces of aqueous vapour at t' and t'' , and p the existing pressure,—I have shown (Trans. R. I. Academy, vol. xvii., p. 285,) that

$$f'' = f' - \frac{t-t'}{87} \times \frac{p-f'}{30}.$$

“In investigating this expression, it is assumed that the specific heat of air, and the caloric of elasticity of aqueous vapour, are constant, and represented (within the ordinary variations of atmospheric temperature and pressure,) the former by .267, the latter by 1115. In subsequently applying this expression to the determination of the specific heats of the gases, (Trans. R. I. Academy, vol. xviii.) it was necessary to give it its most general form, when it was found to become

$$f'' = f' - \frac{48 a (t-t')}{e} \times \frac{p-f'}{30};$$

a being the specific heat of air, and e the latent heat of aqueous vapour, both being supposed at the temperature represented by t' , and under the pressure p . I shall here briefly indicate the steps which conduct to this result. They are given at length in the Philosophical Magazine, for October, 1838.

“The two following propositions constitute the basis of the investigation :

“1st. When, in the case of the wet thermometer, the stationary temperature is attained, the caloric which vaporizes the water, is necessarily equal to that which the surrounding gas evolves in descending through $t - t'$ degrees, *i. e.* from the proper temperature of the air to that of the moistened bulb.

“2dly. The air so cooled, by successive contacts with the moistened bulb, is saturated with humidity.

“From these propositions we easily deduce the equation

$$f'' = f' \left(1 - \frac{m'}{m} \right) \quad (I)$$

in which m' represents the amount of vapour formed by the caloric extricated from a given volume of air, in cooling through $t - t'$ degrees; and m the maximum amount of vapour, which the same volume of air could contain at t' . In this expression f' may be considered as known, the corresponding temperature t' being the result of observation. In order, therefore, to render the formula available, it is only necessary to determine in known terms the values of m' and m .

“ If a be the specific heat of air, and e the caloric of elasticity of aqueous vapour at the temperature t' , it is easy to see that $\frac{e}{a}$ grains of air, in cooling through $t - t'$ degrees, evolve sufficient heat to vaporize exactly $t - t'$ grains of moisture. For m' , therefore, in the formula just given, $t - t'$ may be substituted. Again, m may obviously be replaced by the maximum amount of moisture capable of being contained in $\frac{e}{a}$ grains of air at the temperature t' and pressure p .

But to obtain this, it is only necessary to reduce $\frac{e}{a}$ grains of air to cubic inches; to multiply the resulting volume by $\frac{p}{p - f'}$,* in order to get the expansive effect of moisture; and finally multiply the volume thus obtained by the weight of a single cubic inch of aqueous vapour. When this is done, we find $m = .625 \times \frac{e}{a} \times \frac{f'}{p - f'}$. Reverting now to equa-

* In the investigation given in the Philosophical Magazine, this step is omitted. The omission, however, does not sensibly affect the accuracy of the resulting formula.

tion (I), and writing in it the values just found for m' and m , we arrive at the final equation

$$f'' = f' - \frac{48 a (t - t')}{e} \times \frac{p - f'}{30}, \quad (\text{II})$$

in which the force of vapour at the dew-point is expressed in terms of the force of vapour at t' , and of the difference of the temperatures of the wet and dry thermometers.

“ This formula is applicable for all values of t' above 32° ; but when the stationary temperature of the wet thermometer is lower than the freezing point, it will require modification.

$\frac{e}{a}$ grains of air, we have seen, in cooling through $t - t'$ degrees, convert into vapour $t - t'$ grains of moisture. But if t' be less than 32° , a greater amount of air will be necessary for accomplishing this, inasmuch as the heat evolved has first to liquify ice, and then to convert the water into vapour. The additional quantity is obviously represented by the fraction $\frac{135}{1179 - t'}$; 135 being the caloric of liquidity of water, and $1179 - t'$ the latent heat of aqueous vapour at t' . But this fraction, if we substitute 32° for t' , (which may be always done without sensible error) is equal to 0.118.

Hence for values of t' below 32° , $\frac{e}{a} + 0.118 \frac{e}{a} = 1.118 \frac{e}{a}$ is, in grains, the weight of air which, in cooling through $t - t'$ degrees, vaporizes $t - t'$ grains of moisture. When this correction is applied, the final equation, applicable to observations in which the wet thermometer indicates lower temperatures than 32° , becomes

$$f'' = f' - \frac{43 a (t - t')}{e} \times \frac{p - f'}{30} \quad (\text{III})$$

“ Assuming, as before, the specific heat of air, a , to be .267, the value assigned to it by Delaroche and Berard, and taking for e the value it would have at 50° , upon the hypothesis that 967° is the latent heat of vapour at 212° , and

that the sum of its sensible and latent heat is at every temperature a constant quantity,—equation (II) becomes

$$f'' = f' - \cdot 01135 (t - t') \times \frac{p - f'}{30}; \quad (\text{IV})$$

and equation (III) becomes

$$f'' = f' - \cdot 01017 (t - t') \times \frac{p - f'}{30}. \quad (\text{V})$$

“The theory which has led to these conclusions is now universally admitted to be correct; but as doubts may be entertained respecting the exactness of the coefficient, whose value, as has been seen, depends on the numbers by which a and e are represented, (numbers which are, in all probability, not as yet known with great precision,) it would appear desirable to deduce its value directly from experiment. This is the immediate object of the present communication.

“In my second paper on the dew-point, I have given three distinct series of experiments, applicable to such a purpose;—the first relating to air whose dew-point was determined by Daniell’s instrument; the second to air perfectly dry; and the third to air whose dew-point is known with certainty, and without the aid of any form of condensation hygrometer. From these, in all of which t' is greater than 32° , I have calculated 54 values of the coefficient, by methods to the explanation of which I now proceed.

“1. Representing the coefficient in question by m , the hygrometric formula becomes

$$f'' = f' - m (t - t') \times \frac{p - f'}{30}.$$

Now if air, in reference to which t and t' have been accurately noted, be raised to any higher temperature, and the observation repeated, we obtain data for determining the value

of m . For f'' being constant, $f' - m (t - t') \times \frac{p - f'}{30}$, for

one observation, will be equal to $F' - m (T - T') \times \frac{P - F'}{30}$

for another, from which we deduce

$$m = \frac{(F' - f') 30}{(T - T')(P - F') - (t - t')(p - f')}.$$

“The subjoined tables contain the experiments; and applying to them the method of calculation just explained, we obtain eleven values of m .

SERIES I.

TABLE 1.

No.	t	t'	$t - t'$	p	
1	49.6	44.7	4.9	29.6	} 1 & 2 . $m = .0118$ 1 & 3 $.0121$ 2 & 3 $.0110$
2	88.5	62	26.5	29.6	
3	80.5	59	21.5	29.6	

TABLE 2.

No.	t	t'	$t - t'$	p	
1	47.2	42.5	4.7	30.02	} 1 & 2 . $m = .0144$
2	76	57.5	18.5	30.02	

TABLE 3.

No.	t	t'	$t - t'$	p	
1	48.3	43	5.3	29.76	} 1 & 2 . $m = .0117$ 1 & 3 $.0122$ 1 & 4 $.0132$ 2 & 3 $.0035$ 2 & 4 $.0103$ 3 & 4 $.0109$
2	96	64	32	29.76	
3	91	62.5	28.5	29.76	
4	75	56	18	29.76	

TABLE 4.

No.	t	t'	$t - t'$	p	
1	51.3	45.5	5.8	30.7	} 1 & 2 . $m = .0106$
2	82	59	23	30.7	

Mean value of $m = .01151$

“2. In the general formula given above, if $f'' = 0$,
 $m = \frac{f'}{t - t'} \times \frac{30}{p - f'}$. My observations of t and t' in dry

air, which I here subjoin, enabled me, by means of this expression, to calculate 19 additional values of m .

SERIES 2.

No.	t	t'	$t - t'$	p	m
1	51	33·5	17·5	30·55	·0122
2	53	34·5	18·5	30·35	·0116
3	52	34	18	30·21	·0118
4	51	33	18	30·05	·0115
5	52	33·4	18·6	29·75	·0108
6	53	34·3	18·7	29·50	·0118
7	56·5	35·8	20·7	29·70	·0112
8	58	37	21	29·72	·0110
9	58·2	37	21·2	29·77	·0113
10	58	37	21	30·03	·0114
11	58	37	21	30·15	·0113
12	59	37·5	21·5	30·25	·0112
13	59	38	21	30·26	·0117
14	61	38·7	22·3	30·21	·0113
15	58·3	37·7	20·6	30·35	·0117
16	58	37·5	20·5	30·45	·0117
17	56·3	36·5	19·8	30·30	·0117
18	57·5	37	20·5	30·20	·0116
19	57·5	37	20·5	30·15	·0116
Mean. =					·01150

“3. Lastly, if in the formula f'' and f' be known, so also is m , for it is obviously equal to $\frac{f' - f''}{t - t'} \times \frac{30}{p - f'}$. But in the case of air saturated with humidity, by being passed through water, its temperature is its dew-point; so that this latter is easily and certainly known. Hence, if the temperature of such air be raised, and a wet and dry thermometer be observed in it, we have t , t' and t'' ; and can therefore, by the expression just given, calculate the value of m . The following table includes 24 distinct observations, from which the values of the coefficient given in the last column have been thus deduced:

SERIES 3.

No.	t	t'	$t - t'$	p	t''	m
1	78	62·2	15·8	30·30	51·3	·0110
2	76	61·5	14·5	30·30	51·3	·0111
3	73	60·3	12·7	30·30	51·3	·0108
4	72	60	12	30·30	51·3	·0118
5	69	58·6	10·4	30·30	51·3	·0106
6	90·5	67	23·5	30·15	50·8	·0119
7	82·2	64·3	17·9	30·15	50·9	·0123
8	79	82	17	30·15	50·9	·0110
9	71·7	60	11·7	30·15	51·2	·0116
10	69	58·9	10·1	30·15	51·5	·0112
11	92	69	23	30·42	51·1	·0120
12	83	65·8	17·2	30·42	54·5	·0116
13	76	63·3	12·7	30·42	54·9	·0113
14	68	60·3	7·7	30·42	55	·0112
15	98·5	71·5	27	30·36	55·5	·0117
16	84·6	67	17·6	30·36	56	·0115
17	77·5	64·5	13	30·36	56·3	·0112
18	81	62·2	8·8	30·36	56·5	·0111
19	83	66·5	16·5	30·51	56·8	·0108
20	77	65	12	30·51	57·2	·0117
21	71·3	63	8·3	30·51	57·5	·0116
22	91·8	68·6	23·2	30·51	54·1	·0115
23	75·2	63·2	12	30·51	55	·0116
24	72	62	10	30·51	55·1	·0115
Mean =						·01140

“The following, therefore, are the means deducible from each separate series of observations :

Series 1 . . . $m = \cdot 01151$

Series 2 $\cdot 01150$

Series 3 $\cdot 01140$

So that the mean of all three is $\cdot 01147 = \frac{1}{87\cdot 18}$, or almost exactly the coefficient which I have given in my papers on the dew-point.”

Mr. Lloyd then proceeded to offer some remarks upon Dr. Apjohn's communication, and upon the most probable value of the coefficient to be derived from his results.

We have here, he said, the results of three distinct series of experiments, conducted upon *different principles*, and by *different processes*; and, as we observe, the mean values of the coefficient thus deduced present the most complete agreement, the greatest difference amounting only to ·00011. It is almost indifferent under these circumstances, which of these results be adopted; but in order to do complete justice to the subject, we shall here investigate the *most probable* value of the final mean, as given by the calculus of probabilities.

In order to do this, it is necessary to deduce, in the first instance, the *probable error* of each mean, as derived from the results of its own series. This error, it is well known, is expressed by the formula

$$E = \frac{\cdot455 \Sigma (x - a)^2}{n(n-1)},$$

in which $\Sigma (x - a)^2$ denotes the sum of the squares of the differences of each partial result and the mean, and n the number of observations. The results of this calculation are given in the last column of the annexed Table.

Series.	n	m	E
1	11	·01151	·00031
2	19	·01150	·00005
3	24	·01140	·00006

The most probable value of the final mean, will now be given by the formula

$$m = \frac{\frac{m_1}{E_1^2} + \frac{m_2}{E_2^2} + \frac{m_3}{E_3^2}}{\frac{1}{E_1^2} + \frac{1}{E_2^2} + \frac{1}{E_3^2}};$$

from which we find $m = \cdot01145$.

In the preceding deduction we have supposed that the only errors to which the separate values of m are liable are the errors of observation, in which case the *positive* and

negative errors would be equally probable. But there is another class of errors involved, belonging to the Tables of the elastic force of vapour at different temperatures. In fact, the value of m being expressed in terms of f , and f being calculated from the *observed* value of t , by these Tables, it is obvious that the errors of the Tables will affect the result. In this point of view, however, there is a very important difference between the second series of experiments and the other two. The values of m , in the first and third series, are expressed in terms of the *difference of two values of f* ; so that any *constant* error, in the Tables which give the values of f , must wholly disappear in the result; and any error nearly constant must, for the same reason, be nearly evanescent. The case is different, however, in the second series. Here m is expressed in terms of a single value of f ; and the tabular error of that value has therefore its full effect. Now, that the errors of the Tables are of the kind alluded to,—i. e. nearly constant within certain moderate limits of temperature,—will be evident from the mode in which they are constructed. The value of f is in all cases calculated from an empirical formula, which (within the ordinary range of temperature) does not vary rapidly with moderate changes of t ; the error in the value of f , therefore, (i. e. the difference between its value as calculated with the *assumed* and with the *true* formula,) may, therefore, be regarded as nearly the same, for a moderate range of the variable on which it depends.

It follows from this, that, in the second series, the true probable error is greater than that deduced from the observations themselves, and is the *resultant* of that error and of the error of the Tables. If this latter error were known *à priori*, the resultant error could be inferred; but as this is not the case, we have no means of knowing the *weight* due to the result of that series, and have, therefore, no rule to guide us in combining that result with the other two.

We are thus compelled in strictness to omit that result altogether in deducing the final mean. Combining, therefore, the results of the first and third series, according to the method already laid down, we have

$$m = .01140 ;$$

a result which is identical with that of the third series, that of the first, (on account of its large probable error) not affecting the fifth place of decimals.*

But the second series of experiments, though it cannot properly be combined with the others in deducing the mean, may yet serve another purpose. It may be made, in fact, a test of the accuracy of the different tables of the elastic force of vapour, within the range of temperature belonging to the experiments. With this view, the values of the coefficient, m , have been calculated by Dr. Apjohn from his second series of experiments, by means of three separate tables of the elastic force of vapour. The first of these tables is that which has been employed above, as well as in his papers on the Wet Bulb Hygrometer in the Transactions of the Academy, and is that calculated by Dr. Anderson from the experiments of Dalton and Ure. The second table is that deduced by Mr. Kämtz, from his own experiments; and the third is that given in the Report of the Committee of Physics and Meteorology of the Royal Society, and calculated by Mr. Lubbock from a formula of his own. The results are given in the annexed table.

* The errors of observation in the first series, which are so considerable in comparison with those of the other two, are manifestly owing to the mode of observing f'' . For it is obvious that the rapidly varying temperature of the thermometer in the condensation hygrometer cannot be noted at a precise instant, with the same certainty as that of a thermometer which has arrived at a stationary temperature.

No.	Anderson.	Kämtz.	Lubbock.
1	·0122	·0108	·0110
2	·0116	·0107	·0109
3	·0118	·0107	·0110
4	·0115	·0104	·0107
5	·0108	·0104	·0107
6	·0118	·0107	·0110
7	·0112	·0102	·0105
8	·0110	·0105	·0108
9	·0113	·0104	·0107
10	·0114	·0104	·0107
11	·0113	·0104	·0107
12	·0112	·0103	·0107
13	·0117	·0107	·0111
14	·0113	·0104	·0107
15	·0117	·0108	·0110
16	·0117	·0107	·0110
17	·0117	·0108	·0111
18	·0116	·0106	·0109
19	·0116	·0107	·0109
Mean =		·01150	·01084

It will be remarked at once, on the inspection of these numbers, that the differences of the corresponding results for the *same experiment*, as well as those of the means, are considerably greater than those of *different* results, as calculated by the *same table*: plainly proving that the error due to the imperfection of the tables is greater than the error arising from observation. If we now take the differences between the *mean* values of *m* according to each table, and the final mean already obtained, we find that the error in the value of *m* deduced from the first table is only + .00010. The same error, in the case of the second table, is — .00085; and in that of the third, — .00056. The *probable difference*, supposing the partial means to be affected only by the errors of observation, is less than .00008. We have reason to conclude, therefore, that the second and third of these tables are not so correct as the first—at least for temperatures corresponding to those of the thermometer

with the wettened bulb in these experiments; and that the values which they give for the elastic force of vapour, for these temperatures, are too low.

(*Additional Note by Dr. Apjohn.*)

“M. Kupffer has recently been engaged in discussing the value of m , the coefficient in the hygrometric formula. In a note read by him at the Petersburg Academy, on the 22nd of last January, and published in the *Bulletin Scientifique*, No. 132, after a detailed examination of the experiments of August, Gay-Lussac, Erman, Bohnenberger, and Kämtz, he comes to the conclusion, that the theoretic value of m , or $\frac{48 a}{e}$, agrees sufficiently well with that deduced from the most trust-worthy comparative observations on the dew-point. The formula which he definitively adopts, is

$$f'' = f' - \cdot 267 (t - t');$$

f' and f'' being expressed in tenths of an English inch, and t and t' in degrees of Reaumur's thermometer. But this, expressing f' and f'' in inches, and t and t' in degrees of Fahrenheit's scale, becomes

$$f'' = f' - \cdot 01142 (t - t');$$

an expression in which the coefficient is almost identical with that which has been deduced above from the three series of experiments to which I have so often referred. This formula, however, M. Kupffer observes, gives results in accordance with direct observation, only when the table of the elastic force of vapour drawn up by Kämtz is employed; from which he infers, that *it* alone represents with accuracy the relation between the tension and the temperature of steam—an opinion from which, notwithstanding the high authority of M. Kupffer, I am compelled to differ, on the grounds already stated by Professor Lloyd.

“There is another statement of less importance made by

M. Kupffer, to which also I find it impossible to assent. He alleges that the dew-point obtained directly by Daniell's hygrometer is always lower than the truth; and he ascribes this to the bad conducting power of glass, by reason of which the opposite surfaces of the ball containing the thermometer will, while refrigeration is proceeding, have different temperatures, so that when the outer surface has a dew deposited on it, the temperature of the inner surface, and that of the ether in contact with it, are sensibly lower. I do not deny that, theoretically speaking, this must be the case; but I certainly doubt much whether the cause assigned can produce any appreciable effect of the kind attributed to it. On the contrary, according to my experience, the observed is *almost invariably higher* than the true dew-point. Such must inevitably be the case when the ether is poured on too rapidly; for we have thus a local reduction of temperature at the surface of the ether in the ball containing the thermometer, considerably greater than that indicated by the instrument, as *it* merely shows the mean temperature of the entire column of fluid in which its bulb is immersed. In fact, I have frequently observed, under such circumstances, a ring of dew to be formed, for example, at 44°, and to disappear subsequently, though the temperature of the inner thermometer was kept steadily at this point, or even carried lower,—showing clearly that partial deposition may take place before the true dew-point is attained. The only mode of avoiding this is to pour on the ether very slowly, so as to produce such a gradual lowering of the included thermometer, that the entire of the ether in which it is immersed shall have, at each instant, a temperature which may be considered uniform throughout. As another cause why the observed dew-point is higher than the true, I may mention the augmentation of the humidity of the air in the vicinity of the instrument, by the pulmonary halitus and cutaneous perspiration of the observer; a cause which must

be admitted to exercise a sensible influence, when it is considered how close the observer must be to the instrument, and what a considerable length of time is generally necessary for an observation.

“ While upon this subject I may observe that Professor Daniell’s rule,—to take as the dew-point the *arithmetic mean* between the temperatures indicated by the included thermometer, at the moment of the *deposition* of the ring of moisture, and at the instant of its *disappearance*, appears to me to be erroneous. I have just assigned the reasons which induce me to conclude that the former temperature is (at least in most instances) above the truth; and it is obvious that the latter must always be on the *same side*, for evaporation cannot commence until the temperature of the ball reaches the point of deposition, and will therefore not be completed until it has actually got above this point. The observed results, therefore, being both above the true dew-point, so also will be the mean itself.

“ There is one other topic, suggested by a perusal of M. Kupffer’s note, to which I am anxious to advert. Upon ordinary occasions the dew-point formula may be used without the factor $\frac{p-f'}{30}$, by which it becomes

$$f'' = f' - \cdot 0114 (t - t').$$

This is the form to which it is reduced by M. Kupffer; and though not rigorously exact, the error is generally negligible, within the ordinary variations of atmospheric temperature and pressure. In the case of observations on high mountains, however, it will be indispensable to employ the complete formula, otherwise the calculated dew-point would be appreciably lower than the truth. In illustration of this point, I subjoin the particulars of an observation made on the Sugar-loaf mountain in the vicinity of Bray, the dew-point being experimentally determined by Daniell’s hygrometer, and

calculated by my formula, in its complete and less perfect form, from the observed temperatures of a wet and dry thermometer.

(Top of Sugar-loaf, April 23, 1840.)

$$t = 60^{\circ}8; \quad t' = 53^{\circ}2; \quad t - t' = 7^{\circ}6; \quad p = 28.516.$$

$$t'' \text{ (by Daniell's hygrometer) } = 47.5.$$

$$t'' \left(\text{by formula } f'' = f' - .0114(t - t') \times \frac{p - f'}{30} \right) = 46^{\circ}8.$$

$$t'' \text{ (by formula } f'' = f' - .0114(t - t') \text{) } = 46^{\circ}22.$$

“Thus, by neglecting the factor $\frac{p - f'}{30}$, which, in the preceding observation, = .9366, the calculated dew-point comes out $0^{\circ}58$ too low. This, however, may, under ordinary circumstances, be considered as an extreme error; for $t - t'$ is seldom so high as 7.6 , and $\frac{p - f'}{30}$ scarcely ever so low as .9366, at least in this climate.”

May 25.

REV. H. LLOYD, A. M., Vice-President, in the Chair.

A paper was read by the Secretary, being a continuation of Mr. George J. Knox's researches “on the Direction and Mode of Propagation of the Electric Force, and on the Source of Electrical Development.”

In the commencement of this paper the Author describes some experiments, from which he concludes that *all fluids* convey the electric force through their substance; while with regard to *solids* no regular law exists, some conveying the electric force through their substance, while others convey it along their surface. He next considers the source of electrical development, and shows that it must originate in